

approach allows the use of the whole lumen of the artery for perfusion, thus avoiding high pressure gradients across the cannula. The risk of infection of the vascular prosthesis in cases of longer extracorporeal membrane oxygenation perfusion has to be weighted against potential diameter limitations of the perfusion cannula and complications of the distal perfusion cannula technique (kinking, dislocation, thrombosis). I thank Banfi and colleagues for their interest in our work and their mindful comments.

Stefanos Demertzis, MD  
Department of Cardiac Surgery  
Cardiocentro Ticino  
Lugano, Switzerland  
University of Bern  
Bern, Switzerland

Stefanos Demertzis, MD  
Department of Cardiac Surgery  
Cardiocentro Ticino  
Lugano, Switzerland  
University of Bern  
Bern, Switzerland

## References

1. Demertzis S, Carrel T. Transformation of percutaneous venoarterial extracorporeal membrane oxygenation access to a safe peripheral arterial cannulation. *J Thorac Cardiovasc Surg.* 2013;146:1293-4.
2. Demertzis S, Carrel T. Rapid peripheral arterial cannulation for extracorporeal life support with unimpaired distal perfusion. *J Thorac Cardiovasc Surg.* 2011;141:1080-1.

<http://dx.doi.org/10.1016/j.jtcvs.2014.02.067>

## REPLY TO "IS THE CAUSE OF DEATH IN HYPERTROPHIC CARDIOMYOPATHY LOW PRESSURE GRADIENT IN LEFT VENTRICULAR OUTFLOW?"

### Reply to the Editor:

Kestelli and associates<sup>1</sup> may have misunderstood the analysis in my group's article, "Expanding the Indications for Septal Myectomy in Patients With Hypertrophic Cardiomyopathy: Results of Operation in

Patients With Latent Obstruction."<sup>2</sup> As shown in Table 2 in our original article,<sup>2</sup> preoperative medication use was generally similar between the patients with latent obstruction (gradients of 30 mm Hg or less) and those with high resting gradients (greater than 30 mm Hg). We did not analyze survival according to preoperative drug use, and if, in fact, medications had any substantial impact on late survival, such an effect would likely be related to postoperative medication use rather than to preoperative medication use.

We did not postulate that wall thickness had any impact on survival one way or the other. In our previous studies, however, left ventricular mass and wall thickness were not found to be predictors of late survival after myectomy for hypertrophic obstructive cardiomyopathy, a finding that is in contrast to the results of natural history studies of patients who were not treated surgically.<sup>3</sup>

The apparent minor difference in survival of patients with latent obstruction in our original Figures 2 and 4 is actually due to a slight difference in the number of patients for whom matching was possible in the age-matched and sex-matched population.

The conclusion drawn by Kestelli and associates<sup>1</sup> from the article by Ommen and colleagues<sup>4</sup> is incorrect. In that series, which also came from our clinic, the late survival of patients with obstructive hypertrophic cardiomyopathy who underwent myectomy was similar to the survival of patients who had hypertrophic cardiomyopathy without obstruction. Left ventricular outflow tract obstruction left untreated in patients with hypertrophic cardiomyopathy is associated with reduced late survival,<sup>5</sup> and this situation seems to be improved after myectomy.

Hartzell Schaff, MD  
Division of Thoracic and Cardiovascular Surgery

Mayo Clinic  
Rochester, Minn

## References

1. Kestelli M, Yurekli I, Bademci M. Is the cause of death in hypertrophic cardiomyopathy low pressure gradient in left ventricular outflow [letter]? *J Thorac Cardiovasc Surg.* 2012;144:1537.
2. Schaff HV, Dearani JA, Ommen SR, Sorajja P, Nishimura RA. Expanding the indications for septal myectomy in patients with hypertrophic cardiomyopathy: results of operation in patients with latent obstruction. *J Thorac Cardiovasc Surg.* 2012;143:303-9.
3. Brown ML, Schaff HV, Dearani JA, Li Z, Nishimura RA, Ommen SR. Relationship between left ventricular mass, wall thickness, and survival after subaortic septal myectomy for hypertrophic obstructive cardiomyopathy. *J Thorac Cardiovasc Surg.* 2011;141:439-43.
4. Ommen SR, Maron BJ, Olivetto I, Maron MS, Cecchi F, Betocchi S, et al. Long-term effects of surgical septal myectomy on survival in patients with obstructive hypertrophic cardiomyopathy. *J Am Coll Cardiol.* 2005;46:470-6.
5. Maron MS, Olivetto I, Betocchi S, Casey SA, Lesser JR, Losi MA, et al. Effect of left ventricular outflow tract obstruction on clinical outcome in hypertrophic cardiomyopathy. *N Engl J Med.* 2003;348:295-303.

<http://dx.doi.org/10.1016/j.jtcvs.2014.02.054>

## SHOULD PULMONARY LOBECTOMY BE REPLACED BY SUBLOBAR RESECTION IN PATIENTS WITH STAGE I NON-SMALL CELL LUNG CANCER?

### To the Editor:

We read with interest the article by Altorki and colleagues<sup>1</sup> comparing survival between patients treated by sublobar (SLR) and lobar (LR) resection for clinical stage IA non-small cell lung cancer. SLR is commonly considered only for patients who cannot undergo lobectomy. This practice is based on a randomized trial published by the Lung Cancer Study Group in 1995,<sup>2</sup> which showed LR to be associated with lower rates of local recurrence and cancer-related death. In contrast, Altorki and colleagues<sup>1</sup> found equivalent survival between SLR and LR in clinical stage IA. We congratulate them for this study

and believe that in the future their conclusions could radically change and enlarge the role of SLR in the treatment of patients with non-small cell lung cancer.

Our most important concern, however, lies in the cases of unexpected N1 or N2 disease, ranging from 4% to 7% in the experience of Altorki and colleagues<sup>1</sup> and reported at similar levels in the most recent literature. In this group of patients with disease upstaged to stage II or III, SLR is not indicated and LR is certainly the optimal treatment.<sup>3</sup>

In addition, we would like to focus the attention on some features that could limit the meaning of the study. First, their results are based on a population enrolled in the International Early Lung Cancer Action Program (I-ELCAP) group according to specific criteria.<sup>4</sup> This very uniform population may not represent the general population of a daily clinical practice. Second, this was not a randomized study, and the criteria used by the different surgeons to decide whether to perform LR or SLR and the surgical approach adopted are not reported. Third, there were some patients in both groups who did not undergo a complete nodal sampling. This could lead to incorrect staging and influence survival results. Finally, in our experience and according to several studies, wedge resection is associated with a higher rate of recurrence than is segmentectomy. This gap could be determined by several factors, such as smaller parenchymal margin and lower yield of lymph nodes.<sup>5</sup> Also, Altorki and colleagues<sup>1</sup> in their study reported that segmentectomy was associated with a lower recurrence rate than was wedge resection. Despite this, LR and wedge SRL have the same survival. We underline that wedge resection and segmentectomy are not oncologically equivalent and suggest that they be considered separately.

In conclusion, Altorki and colleagues<sup>1</sup> report interesting data, but

the topic might be better investigated with a randomized study to draw definitive conclusions.

Alessandro Baisi, MD  
Matilde De Simone, MD, PhD  
Ugo Cioffi, MD, PhD  
Federico Raveglia, MD, PhD  
Thoracic Surgery Unit  
Ospedale San Paolo  
University of Milan  
Milan, Italy

## References

1. Altorki NK, Yip R, Hanaoka T, Bauer T, Aye R, Kohman L, et al. Sublobar resection is equivalent to lobectomy for clinical stage IA lung cancer in solid nodules. *J Thorac Cardiovasc Surg*. 2014; 147:754-62; discussion 762-4.
2. Ginsberg RJ, Rubinstein LV. Randomized trial of lobectomy versus limited resection for T1 N0 non-small cell lung cancer. Lung Cancer Study Group. *Ann Thorac Surg*. 1995;60:615-22; discussion 622-3.
3. Baisi A, De Simone M, Cioffi U. Clinical implications related to preoperative detection of stage IA lung adenocarcinoma [letter]. *J Thorac Cardiovasc Surg*. 2013;145:1148.
4. Henschke CI, Yankelevitz DF, Smith JP, Miettinen OS, ELCAP Group. Screening for lung cancer: the early lung cancer action approach. *Lung Cancer*. 2002;35:143-8.
5. Kent M, Landreneau R, Mandrekar S, Hillman S, Nichols F, Jones D, et al. Segmentectomy versus wedge resection for non-small cell lung cancer in high-risk operable patients. *Ann Thorac Surg*. 2013;96:1747-54; discussion 1754-5.

<http://dx.doi.org/10.1016/j.jtcvs.2014.02.070>

## RELATIVE AMPLITUDE INDEX: A NEW TOOL FOR HEMODYNAMIC EVALUATION OF PERIPROSTHETIC REGURGITATION AFTER TRANSCATHETER VALVE IMPLANTATION

### To the Editor:

Heinz and colleagues<sup>1</sup> present an interesting concept to help clinicians assess the severity of paravalvular leakage (PVL) after transaortic valve insertion; however, their report raises a number of issues.

The association of PVL severity as assessed by echocardiography and poor outcomes after transcatheter aortic valve implantation is well known.<sup>2</sup> It will be difficult to adopt

the relative amplitude index (RAI) as a new marker compared with an echocardiographic assessment, because the authors have not presented data on the outcomes for patients with a mild PVL on the echocardiogram and a high RAI versus severe PVL and a low RAI.

With regard to the statistical analysis, no correlation coefficient was presented for PVL severity and RAI. The PVL severity and RAI are almost certainly related; however, no interaction analysis was performed for mortality or long-term survival. The univariate analysis of death did not include age, Agatston score, or annular eccentricity as covariates, probably the most important factors in determining PVL severity.<sup>3</sup> In addition, perioperative respiratory failure had the greatest odds ratio for death, but preoperative chronic obstructive pulmonary disease was not significant, implying that technical issues at implantation could be an issue. No correlation between the RAI and perioperative complications were presented. An RAI cutoff value of 14 was deduced from the receiver operating curve analysis; however, no sensitivity or specificity data were presented. Also, only 7 of the 110 patients in the cohort had an RAI of  $\geq 14$ , suggesting general applicability might be an issue. With regard to mortality, we question their finding of a significant difference ( $P = .013$ , Table 2), because we calculated the mortality difference as nonsignificant ( $P = .1$ ).

The formula presented for the derivation of RAI was not referenced or derived in their report, and, despite the simplicity, we believe, from a mathematical and engineering viewpoint, is incorrect, according to previous reports.<sup>4,5</sup>

$$BP_{d-pre} = F_d \cdot SVR_{pre} \quad (1)$$